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have at any time been admitted to a degree in the University of Cambridge. The value of the prize is about £220. The essays must be sent to the vice-chancellor on or before the last day of December, 1914.

#### UNIVERSITY AND EDUCATIONAL NEWS

IN memory of a husband who for years had suffered from a malady that eluded medical skill, Mrs. George William Hooper, of San Francisco, has transferred to the University of California \$1,000,000 for the establishment of an institute of medical research. The foundation is to be controlled by an advisory board of seven members constituted as follows: The president of the Carnegie Foundation, who is now Dr. Pritchett; the professor of pathology at the Johns Hopkins University, the director of the Rockefeller Institute for Medical Research, the president of the University of California, the dean of the Medical School of the University of California, E. D. Connolly, representing Mrs. Hooper, and a seventh member to be chosen by the western members of the advisory board.

THE late Dr. Louis A. Duhring, formerly professor in the University of Pennsylvania, in his will disposes of an estate valued at about \$500,000. His notes on medical cases are given to the university, and the will creates a trust fund of \$25,000, the income of which is to be used for the benefit of the department of cutaneous medicine. The will gives the University of Pennsylvania Hospital \$50,000 for the establishment of free beds in which cutaneous, cancerous and allied diseases shall be treated. After making a number of private bequests, the testator directs that the residue of the estate be given to the trustees of the University of Pennsylvania, and that it be applied to the treatment of cutaneous diseases and their study.

MR. STEVENS HECKSHER has given \$10,000 to the University of Pennsylvania to establish a fellowship in medical research.

FRIENDS of Professor William Otis Crosby have presented to Columbia University the sum of \$1,800 for the establishment of a collection of lantern slides to be known as the

"William Otis Crosby Collection of Geological Lantern Slides."

THE trustees of the University of Illinois have voted that for students entering in September, 1913, the requirements for admission to the College of Medicine (formerly the College of Physicians and Surgeons of Chicago) be raised to at least one year of collegiate work in addition to fifteen units of common and high school work, and that for students entering September, 1914, the minimum requirement further be increased to two years of collegiate work in some college or university of recognized standing.

MR. C. L. DAKE has been appointed assistant professor of geology and mineralogy in the Missouri School of Mines. He was instructor in geology at the University of Wisconsin during 1911-12, and during 1912-13 at Williams College.

DR. LEO F. GUTTMANN, formerly head of the division of physical chemistry at the College of the City of New York, and for the last four years assistant professor of physical and industrial chemistry at Queen's University, Kingston, has been appointed associate professor of chemical engineering.

DR. GEORGE SHANNON FORBES, instructor in Harvard University, has been promoted to be assistant professor of chemistry.

PROFESSOR OSWALD KÜLPE succeeds Professor Th. Lipps as professor of philosophy at Munich.

#### DISCUSSION AND CORRESPONDENCE

##### THE LAWS OF NOMENCLATURE IN PALEONTOLOGY

TO THE EDITOR OF SCIENCE: A number of recent letters in SCIENCE on the subject of nomenclature may serve as an excuse to present to those interested a few of the special difficulties that beset the vertebrate paleontologist in questions of nomenclature.

The writer holds no brief for the law of priority. Names, scientific or popular, are, after all, but words designed to convey a certain concept, and the fixity and uniformity of that concept might quite well have been—or be—secured by an official dictionary which

should do for scientific names what the standard dictionaries do for words in general, namely, embody and fix as accurately as possible the current usage and significance of the word. That is what the proposal for an official list of scientific names amounts to, if carried out to the limit of its apparent trend. It would no doubt substitute references to types or descriptions for the dictionary definitions of meanings; but that is unessential.

The real objections to such a plan, as it seems to me, are (1) that the law of priority is so thoroughly imbedded in the mind of most systematists, and regarded so much as a moral or legal issue, a matter of justice to the first describer or of correct interpretation of certain statutory rules, rather than as a matter of convenience, that the authorities in systematic work would not abide by dictionary usage in the matter. (2) That any extensive list that could be prepared would certainly contain many names that were open to exception because the references or types so standardized were inadequate, or the current usage not approximately universal.

The first objection is illustrated by Dr. Dall's contemptuous rejection of the proposition that proposals approved by the majority of the committee on nomenclature should be submitted for endorsement or rejection to the body of the Zoological Congress. He will not abide by majority rule in the matter, even a majority of a committee of experts; and for a majority decision of "five dollar subscribers" he has no respect at all. The second objection is one that would be of special weight in any attempt to standardize the nomenclature of vertebrate paleontology.

But it has been said: If the systematists will not conform, let them go their way, and the rest of us go ours. To such a remark one can only say: Try to put such a scheme in the form of a definite program and see where it would land you. The scientific body is an organic whole interacting in all its parts, and *Æsop's* fable of the belly and the members is very much apropos. Altogether it would seem that the present methods and usages, annoying and exasperating as they often are to the

teacher and morphologist, wasteful and time-consuming to the systematist, can not be modified to any material extent without causing further confusion.

The systematists are in the habit of assuring us that this confusion is only temporary; that when the laws of priority have been correctly and exactly applied to all species and genera, a stable and unchanging nomenclature will result; there will be no further changes. So far as vertebrate paleontology is concerned, I am certain that this optimism is unjustified and I doubt whether it is so in other branches of zoology. After the nomenclature has been revised it will be stable until somebody revises it again, just so long and no longer. Every new reviser, having new evidence at hand, or stressing differently the data already considered, is liable to interpret the case differently, and each difference in the interpretation of some obscure or minor point is liable to result in a whole series of alterations of well-known and important names. Only by forbidding the re-investigation of cases already authoritatively considered can changes be prevented. And that is just what Professor Ward's committee wants to do, and Dr. Dall makes it clear that systematists of the highest standing would not accept any such ruling. The plain fact of the case is that scientific nomenclature has come to a pass where the common name of a species is the only name with any permanency or prospects of permanency, and it is necessary to use it or to provide one if there is none already, in order that one's readers—aye, even other systematists—shall know what animal is under discussion. A century ago scientific writers wrote descriptions in dog-latin and then explained in good English or other modern languages what they were talking about. To-day they write descriptions under a scientific name dug out of some old forgotten treatise, and provide it with a wealth of learned synonymy, and then explain by means of the "dear old familiar name of the text-books" or the still dearer vernacular name, what animal it is that they are describing. Fashions change; not always in the way of progress.

In his last letter, Dr. Dall suggests a method of reconciling the differences between teachers and systematists by allowing the use of the "text-book name" with the status of a vernacular name, and a plus sign before it. I adopted a somewhat similar compromise some years ago, in a check list of American Tertiary mammals, only I put the commonly accepted name first, and the "correct" name afterwards, enclosed in brackets and with an equality sign before it. Now doubtless there are specific differences between this and Dr. Dall's discovery, but I claim that the *genus* is the same, and that therefore, *I* am entitled according to the law of priority of which he is so able a defender, to that statue which he expects to receive from the grateful teachers. Especially as I am sure my modification would be more acceptable to them, and while I feel less certain of his cordial approval, I don't see what legitimate exception he can take to it.

The vertebrate paleontologist is in some respects almost free from the difficulties in interpreting and applying the laws of nomenclature that beset his zoological brother. The literature with which he deals is mostly of recent date, and reasonably cognizant of the laws and decencies of nomenclature. There are only a few cases in vertebrate paleontology where there is any particular difficulty in fixing the type of a genus, the date of its publication or the species intended to be included under it.

His serious problem lies in the nomenclature of species, the identification of type specimens, and especially to know what to do with the fragmentary and almost indeterminate types of most of the older and many of the newer species, in relation to more complete specimens subsequently obtained. A quotation from Professor Marsh may be apropos.

A single tooth or vertebra may be the first specimen brought to light in a new region and thus become the sole representative of a supposed new form. The next explorer may find more perfect fragments of the same or similar forms, and add new names to the category. A third investigator with better opportunities and more knowledge may perhaps secure entire skulls or even

skeletons from the same horizon, and thus lay a sure foundation for a knowledge of the fauna.<sup>1</sup>

The wording is curiously suggestive of Professor Marsh's probable opinion of the activities of Leidy, Cope and himself in the field of American paleontology; but it is at all events a sufficiently accurate description of the general progress of the science. The earliest finds in any newly explored formation are generally fragments. They are new, they are of scientific importance, they are distinct from forms hitherto known, they ought to be described and figured, and they ought to be named as a matter of convenience in scientific discussion. But they will undoubtedly make trouble later for the systematist. The "next explorer" must either "add new names to the category" or identify one or more of his fragments with the first described type. And if his material comes from a different locality such identifications may cause serious errors in stratigraphic correlation. The third investigator may ignore the earlier types as too incomplete for identification, or he may arbitrarily identify them with such of the species secured by him as suits his convenience. Either method will subject him to criticism and be liable to mar the scientific results of his investigations.

It is a covenant universally accepted that a new species is not to be described unless it can be shown or inferred to be different from all previously described species. But here it simply can not be applied. The third investigator may have at hand skulls and skeletons of a dozen species all clearly distinct from one another, yet any one of them may be conspecific with the tooth or fragment on which an earlier species was founded, and it is often absolutely impossible to find in the type any characters that are really valid evidence for referring to it one rather than another of these later discovered species.

The difficulty in treating of these more or less indeterminate species recurs again and again in the literature of vertebrate paleontology, causing endless confusion and error when arbitrary identifications are subsequently

<sup>1</sup>"The Value of Type Specimens," *Am. Jour. Sci.*, 1898, Vol. VI., p. 402.

found erroneous and infinite recrimination and heartburning when the work of earlier authors is set aside or ignored. These troubles we shall have with us always; but perhaps their amount might be reduced if an intermediate course were adopted.

The earlier type may be a specimen showing unmistakable ordinal, family or generic characters, but not adequate as a specific type. Let it stand so. Do not set it aside as "indeterminate," but specify the extent to which it is determinable. It can remain in the literature and be included if desirable in faunal lists, but additional material should not be referred to it unless the new specimens be topotypes, *i. e.*, from the same locality and the same geological level, so far as these are recorded or can be safely inferred from the literature, unpublished notes or labels or the appearance of the specimen. If it has valid generic characters a genus founded upon it is valid, and other species may be referred to it; if it has family characters but no distinctive generic characters, a family name founded on the genus is valid, but no subsequent genera are to be synonymized with it except when species of those genera are known to occur in the locality and geological horizon of the older genotype species. In illustration a few cases may be cited:

1. *Anchippodus riparius* Leidy 1868, type a lower molar from the "Miocene" (? Oligocene) of New Jersey. Type of the family Anchippodontidæ Gill 1872, referred to the order Tillodontia Marsh 1875. Leidy referred to this genus and species in 1873, his *Trogosus castoridens* 1871 based on a lower jaw from the Middle Eocene (Bridger formation) of Wyoming and to the same genus Marsh's *Palæosyops minor* 1871, based on a lower molar. Marsh, subsequently obtaining complete skulls and skeletons of related animals, accepted Leidy's genus *Anchippodus*, described a new genus *Tillotherium* with three new species, and based upon it the family Tillotheridæ which he made typical of the order Tillodontia.

No topotypes of *Anchippodus riparius* are known. Subsequent authors have either fol-

lowed Marsh in ignoring Gill's name, while accepting Leidy's identification of *Anchippodus* with *Trogosus*, and considering *Tillotherium* as distinct from the latter, or they have used Anchippodontidæ as the family name, while deriving all the characters of the group from Bridger materials.

The result is that the faunal lists record in the New Jersey "Miocene" along with a known Oligocene mammalian fauna (*Cænopus*, *Entelodon*, *Protapirus*) a genus and species of the Middle Eocene fauna, while the western collections make it reasonably certain that in those regions the family and order disappeared with the Middle Eocene. Were this conclusion supported by real evidence, it would lead to some interesting corollaries as to migration and survival. In fact it is quite misleading. The type of *Anchippodus riparius* is inadequate for specific or generic comparison, and doubtfully adequate for family or ordinal comparison. It is very improbable that it is congeneric with *Trogosus*, hardly possible that it is co-specific with *T. castoridens*, so far as one may judge from the associated fauna in absence of generic or specific characters in the type specimen. Gill's family characters were drawn from *Trogosus*, and since it is doubtful whether this genus belongs in the same family with *Anchippodus*, his family should be held as doubtfully synonymous with Tillotheriidae, both names to be retained, but the former as " ?Anchippodontidæ Gill, fam. indet."

2. *Hippodon* Leidy is the first genus of three-toed horses described from this country. The type is *H. speciosus*, based upon a lower molar tooth. Leidy subsequently referred to the species upper teeth, etc., which he considered congeneric with the older European genus *Hipparion*. On the basis of these and other referred specimens the species was held valid and the genus a synonym of *Hipparion* until Gidley revised the three-toed horses in 1907. No topotypes were or are known. Gidley set aside both genus and species as indeterminate. I subsequently identified and located the type specimen which had been missing, and after making a fairly careful com-

parison came to the conclusion that it was a species of *Merychippus*. A more thorough restudy of the Miocene horses last summer brought me to the conclusion that this tooth, while certainly distinct from *Hipparion*, lies somewhere near the border line between *Merychippus* and *Protohippus*, but on which side of the line I can not determine except arbitrarily. The species is, therefore, in fact indeterminate generically, and a valid genus can not be based upon it. *Hippodon* would, however, stand as the type of a group including *Merychippus*, *Protohippus* and *Pliohippus* as contrasted with *Hipparion* and *Neohipparion*. In stratigraphic correlation of the beds at Bijou Hill, where it was found, it would be listed under the *Protohippinæ* as *Hippodon speciosus* gen. et sp. indet.

3. *Deinodon* Leidy is determinable as to family, but is not determinable generically, as the genera of carnivorous dinosaurs are now distinguished. The same is true of a whole series of genera and species described by Leidy and Cope from the Judith River. The treatment of types and referred specimens of these genera by paleontologists as specifically distinguishable or identical has sadly misled Dr. Peale in his recent discussion of the vertebrate evidence as to the age of the Judith River beds, leading him to present as conclusive evidence of identity in age a correspondence in fauna which to those who know the nature of the specimens on which the lists are based is no evidence at all.

In brief the plea is for the full recognition of nomenclature laws, but for the avoidance of arbitrary or unprovable identifications in the future, and the recognition of the actual facts as to the extent to which described genera and species are truly determinable. The allowed exception in the case of topotypes is based upon an inference of identity which it would seem impossible ever to prove incorrect. In all other cases the chances that future discovery may upset an arbitrary identification should prevent its being used as a basis for changes in nomenclature.

The source of the present lamentable situation in nomenclature is that an excellent sys-

tem of procedure, designed to settle unsettled questions, has been wrenched from its intent and used to unsettle settled questions. The present writer, having studied with more or less care the majority of the type specimens of American fossil mammals and reptiles, has abundant evidence at his command to upset by a strict application of the accepted laws and procedures, much of the present nomenclature, including many of the alterations proposed in recent years upon grounds of priority. But he has no intention of so misusing his opportunities, or of being responsible for such changes until convinced that they will really result in greater stability.

W. D. MATTHEW

#### HOW IS THE WORD FOOD TO BE DEFINED?

THE query expressed in the title "How is the word 'food' to be defined?" is suggested by a restrictive usage of this word which is rather prevalent in American text-books of elementary botany, and which seems to have originated among American plant physiologists. Presumably it had its birth in university courses in botany where the arguments for its use were given and understood, but as it appears in the elementary texts, it involves a marked inconsistency of thought and expression for which no provision is made. Since it represents a striking divergence from the ordinary meaning of the term "food," it deserves wider consideration, looking either toward its general adoption, if desirable, or else toward its discontinuance.

The word food, according to its ordinary connotation, is applied to any substance which, when taken into the body of an organism, can be used by that organism in the construction of new tissue. Definitions of essentially this content are to be found in the Century, Standard and Webster dictionaries. Using this definition as a basis, we should consider as food for green plants the water, carbon dioxide and mineral salts absorbed from the surroundings. According, however, to the restricted usage, these are not considered as "foods," but are referred to as "raw materials," "nutrients," "food materials," or some other cir-